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THREE LECTURES

DELIVERED AT THE

HOSPITAL FOR SICK CHILDREN,

GREAT ORMOND STREET,

1886.

BY ROBERT J. LEE, M.D., F.R.C.P.

LECTURES I AND II.

ON THE TRANSMISSION OF SYPHILIS; with Cases arranged to illustrate the relations between the various Symptoms of Hereditary Syphilis in Children and the Parental History.

LECTURE III.

ON THE EARLIEST RECORD OF WHOOPING-COUGH.

LONDON:
BAILLIÈRE, TINDALL, & COX,
KING WILLIAM STREET, STRAND.
1886.

Price One Shilling.

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LECTURE I.

ON

THE TRANSMISSION OF SYPHILIS.

GENTLEMEN,—Having explained to you the method which should be followed in the clinical study of infantile or transmitted syphilis, I propose now to analyse some of the cases to which reference has been made in former lectures. (*a*)

We have divided and classified the facts in the history of each case in the following manner:—

Column I of our table contains a number or name for the purpose of reference.

In Column II is noted the period of time between infection of the parent and marriage.

In Column III is noted the interval of time between marriage and the date when the case has come under observation.

In Column IV is indicated, by means of a symbol, whether the mother was primarily infected, or was infected by conception after marriage. How this occurs I have already explained.

In Column V we register details of the father's history.

In Column VI details of the mother's history.

'Lectures on the Diseases of Children." Second Edition. London: Baillière, Tindall & Cox.

And in Column VII all facts relating to the offspring, including the results of each conception.

The symbol used in Column IV is S_1 or S_2 ; S_1 indicating that the mother has been primarily infected ; S_2 that she was infected through conception, or at least after marriage, and not primarily. Let me illustrate this by the two examples.

Cases.	II.	III.	IV.	V.	VI.	VII.
A. B.	4mo.	10yrs.	S_1	Æt. 36. Four months before marriage was infected. The sore healed, but broke out again after marriage.	Æt. 34. Infected soon after marriage. Had no secondaries.	1st born dead. 2nd born dead at 7 mo. 3rd born alive at 8 mo. lived 3 yrs. 4th born alive at 9 mo., lived 6 weeks. 5th alive, under treatment at this hospital. 6th alive and healthy. 7th born alive, died while under treatment here for periosteal disease, abscess, &c.
C. D.	m	4y.	S_2	Æt. 36. 12 months before marriage had gonorrhœa followed by rash, in 4-5 mo. Well at time of marriage.	Æt. 28. No symptoms at any time.	1st born dead. 2nd alive, now æt. 2, was in same condition as this child when 4 mo. old. 3rd present, æt. 4 mo. Ulcers on nates; diffuse inflammation of left leg. Had snuffles soon after birth.

Now, let me explain the purpose of this arrangement. You must have observed that the signs of syphilis vary greatly in infancy. You will find, on inquiry, that this depends upon the interval of time between the infection of the father and the conception by the mother. This remark applies to those cases where the father has recovered from primary symptoms at the time of marriage. You see, therefore, the importance of Column II. It enables you to determine at once the question of time referred to.

You doubtless are acquainted with the fact that in the family history of syphilis, each succeeding conception has better prospect of development than the last; and each succeeding infant shows diminished evidence of infection. By Column III then you can determine at once the length of the married life of the parents.

I need hardly say anything about Column IV, except that it enables you when the history is clear, to separate S_1 cases from S_2 cases. There is one remark, however, which may as well be made now in regard to this column. It is this—that you will sometimes have a doubt as to the way in which the mother was infected, and you are obliged to put a query after the symbol. These cases deserve very careful consideration, and I shall refer to them later on. Let us indicate them by $\left(\frac{S_1}{S_2}\right)$, for the purposes of reference. No remarks need be made concerning Columns V, VI, VII, as they explain themselves.

Let me say a word or two on this tabulation of cases. One has to sacrifice much that is important and interesting by the limitations imposed by this formal system; but there is this advantage that it assists us very greatly in the object we have in view, namely, the application of the inductive method to determine certain laws or principles, and chiefly by our data being arranged in such a way as to help us to distinguish what are constant from what are variables. No case must be admitted into such a table unless it is accurately recorded; and I do not mean by *accurate* that the facts stated be *true;* but the facts must be complete. An omission becomes a possible source of error. We must admit all possibilities in this inquiry, must disregard all theories and opinions, and authority, and simply leave the mind perfectly free to follow the path that is indicated by the phenomena before us.

We have, therefore, three classes of cases to deal with: (S_1), (S^2), and $\left(\frac{S_1}{S_2}\right)$ and you will readily appreciate the points of difference between them. In cases of S_1 the mother has been primarily infected, and the condition of the father is not of much importance. What happens under such circumstances has long been known to our profession, and, as a class, these cases have not presented the same interest as the two other classes. I propose, therefore, to take the S_1 cases last.

The first point to which I would direct your attention in the study of S_2 cases, is the singular frequency of the note in Column VI that the mother has shown *no symptoms of infection.* You are aware that we begin the study of each case in hospital practice with the infant or child; and as the mother herself usually brings her infant, we have the opportunity of ascertaining all the details necessary for Columns VI and VII. It is well, therefore, to clear your minds from any opinion you may have formed, that you will probably obtain some evidence of maternal troubles in this class of cases. You might imagine that the mothers were inclined to conceal their histories, much in the same way that men do when you touch upon this delicate subject. Married women will deceive you when they have been primarily infected previous to marriage; but they have no disposition to do so when they have a clear conscience. If they have been infected subsequent to marriage by the husband you will easily ascertain the fact, and, therefore, be careful not to push inquiry in a thoughtless way, and excite suspicions which might cause family discord.

It is of the greatest importance then in the study of transmitted syphilis to bear in mind that in the histories presented to our consideration in hospital and private practice, it is quite unusual to have any evidence of maternal infection. Let us separate those cases of S_2 where the mother has never shown symptoms of infection from the rare cases where she does show or has shown them, and let us arrange some of the cases of the first kind in the following tabular form.

Col. I.		III.	IV.	V.	VI.	VII.
1.	12m	3y	S	Father's History. — Æt. 29. Twelve months before marriage under treatment at Dean Street. Eruption 3 m. later, and again 3 m. before birth of last child.	Mother's History. Æt. 24. 0	Family. 1. Born dead. 2. Healthy till 18 m. old; then vag. discharge and otorrhœa for 1 m. Since well. 3. Present. Æt. 3 m. Syph. blebs, varying from a pin's head to half-a-crown. Snuffles. Wasting.
2.		9y	S_3	Æt. 37. Under treatment at King's Coll. Hos. Eruption 6 m. later.	Æt. 34. 0	1. Born dead. 2. Lived 6 m. 3. Lived 10 days. 4. Is 7 years old. Strong, healthy. 5. Under me in 1874 with syph. furuncles. 6. Now 3 y. old. Healthy.
3.	?	H_1 15y H_2 5y		First husband married 15 years ago. Dead 5 yrs. after. Second husband married 5 years ago. Healthy.	Æt. 33. 0	Children by husband 1st— 1. Born dead. 2. Lived 3 m. 3. Born dead. 4. Born dead. Seven years passed between birth of 4 and 5. 5. (1st by 2nd husband) at 14 weeks syph. cachexia. Snuffles. Wasting.
4.	6y	6y	S_2	Æt. 30. Under Mr. Shillitoe. Now has ulceration of nasal bones.	Æt. 29. 0	1. Alive and well. 2. do. do. 3. Died tuberc. meningitis. 4. Alive and well.
5.	11y	3y	S_2	Æt. 36. Under Mr. Coulson 14 years ago. No secondary symptoms.	Æt. 26. 0	1. Died at 7 m. 2. Syph. psoriasis of hands and feet.
6.	9m	2¼y	S_2	Æt. 29. Dean Street for 2 m. No socondaries.	Æt. 23. 0	1. Æt. 14 weeks. General syph dermatitis. Snuffles. Ulceration of nates
7.	3y	6y	S_2	Æt. 29. Under treatment at St.	Æt. 26. 0	1. Alive and well. 2. do. do.

Col. I.	II.	III.	IV.	V.	VI.	VII.
				Barthol. Hosp. Came voluntarily.		3. Alive and well 4. Eczema of nates, not specific.
8.	2y	2½y	S_2	Æt. 27. Treated at Manchester for primary. "Infected 2nd time ten months later."	Æt. 26. 0	1. Was under Dr. D. Lees. 2. Syph. dermatitis round nose. Snuffles. Psoriasis of hands and feet.
9.	?	9y	S_2	First husband died in Brookwood Asylum 3 years ago. 2nd husband healthy.	Æt. 35. 0	1st Husband. 1. Died at 4m. 2. Born dead. 3. Born dead (premat. 7m.). 4. Born dead (premat. 8m.). 6. Alive. "Delicate." 2nd Husb. 7. Æt. 2¾. Syph. condyloma. 8. At 11m. Healthy.
10.	1¾y	7y	S_2	Æt. 30. Primary. No secondaries.	Æt. 25. 0	1. Defective nutrition. Cachectic. Nothing definite.
11.	6m	7y	S_2	Æt. 33. Had primary when in army. No secondaries.	Æt. 28. 0	1. Alive and well. 2. Miscarriage. 3. Alive and well. 4. At 8m. An erythema of purple tint, later scaly; on face first, then thighs and buttocks.
12.	1y	2½y	S_2	Æt. 23. Primary and buboes. St. Mary's. Sore throat and rash.	Æt. 20. 0	1. Died at 7m. Rash soon after birth. 2. Premature at 7m. Syph. dermatitis, snuffles, &c., soon after birth.
13.	3½y	4½y	S_2	Æt. 29. French Hospital for 7m. Bubo. No sore throat [or eruption.	Æt. 26. Scrofulous scars. 0	1. Miscarriage. 2. do. 3. Died six day and premat. 7m.

Col. I.	II.	III.	IV.	V.	VI.	VII.
13.	3½y	4½y	S_2			4. Died third day and premat. 8m. 5. Premat. 7m. child. Æt. 5w. Snuffles, waxy, syph. periostitis.
14.	2y	4y	S_2	Æt. 28. Distinct history of primary.	Æt. 23. 0 ?	1. Æt. 4m. Snuffles, coppery spots on nates and abdomen.
15.	8y (*a*)	20y (*a*)	S_2	Æt. 51. Infected 28 years ago, twice salivated before 1st marriage. Lost first wife (*a*) 8 years after.	*a.* 0 1st wife.	Children by (*a*): 1. Premature. Died. 2. Alive and well. Æt. 17 years. 3. Died. Premature. 4. Died. Premature.
	18y (*b*)	10y (*b*)		Widower 5-6 years, then married second wife. (*b*)	*b.* 0 2nd wife.	Children by (*b*): 1. Born dead. 2. Born dead. 8m. 3. Born dead. 7m. 4. Born dead. 7m. 5. Twins. Born dead. 7m. 6. Alive and well. Born at term. Now two years old. 7. Present. Æt. 3 months. Hands and feet peeling; snuffles; periostitis.
16.	6y	3y	S_2	Æt. 26. Infected 6 years before marriage. No secondaries.	Æt. 28.	1. Æt. 27. Small syphilitic furuncles on left leg, calf, and neck, also meta-carpal of left thumb.
17.	11m	4y	S_2	Æt. 36 "Gonorrhœa" followed by rash	Æt. 27. 0	1. Born dead. 2. Alive, now æt. 27, was like No. 3 when 4m. old. 3. Present. Æt. 4m. Ulcers of nates, diffuse periosteal swelling of left leg. Snuffles soon after birth,

Col. I.		III.	IV.	V.	VI.	VII.
	3y	6y	S_2	Æt. 31. No secondaries.	Æt. 27. 0	1. Lived till 9 weeks. 2. Born dead. 3. Born dead, 7 m. child. 4. Miscarriage. 5. Lived 6 wks. 6. Present. Æt. 4m. Well till a week ago. Numerous purple furuncles thickly over all parts of body.
19.	2y	10y	S_2	Æt. 45. Bubo, gonorrhœa, secondaries lasting a year.	Æt. 33. 0	1. Died at 10½ months. 2. Died at 5m., whooping-cough. 3. Died at 10 m., measles. 4. Died at 1 y., measles. 5. Alive, æt. 17, delicate. 6. Present, snuffled from birth. Born with cough. Discharge from right nostril. Left arm and periosteum much swollen; distinctly specific.

In hospital practice the history of a case starts from the infant, and step by step we have to go back through the series of phenomena or processes that have transpired between the conditions before us, and the very earliest stage of the experiment, namely the infection of the parent. But in family practice it is of common occurrence for the medical adviser to know the antecedents of the parents, and he can anticipate the transmission of syphilis without waiting for evidence in the offspring.

For this reason there are very few cases of family syphilis in our table which have started from the parental history. In a former lecture I referred you to the large number of cases mentioned by Fournier, where both the mother and the offspring remained healthy; and I briefly stated under what conditions this occurs. They form a most important class of cases, and must be carefully considered in their proper place.

You will see from the annual report of this hospital that between 250 and 300 cases of syphilis, out of a total of about 12,000, pass through the out-patient department every year, and are fairly distributed among the attending physicians and surgeons. It is only under special circumstances, and by special arrangements, that we can collect all the details necessary for complete histories. I have mentioned this in order to show you to what extent the opportunities of private practice, as is well known, exceed those afforded by hospitals, and how well you are able in private practice to study this very interesting and important matter for yourselves.

The first remark one would make about the children referred to in these cases of S_2 is that they showed the symptoms in a comparatively mild form. If you compare the infant whose mother was primarily infected at the time she conceived, with one of these, the difference is striking. We should admit then that, as regards the infant, the cases of S_2 where the mothers have not suffered, are slight cases. You will see presently that cases of S_2 where the mothers suffered are more serious ; but we cannot draw any line between the infantile conditions and separate them into two or three classes. The infant or young child may have syphilis in any degree; and what we want to determine is the relation between the child and parent. It must be quite clear to you that generally the condition of the parent is the determinant of the condition of the child. Our next point then is to compare Columns V and VII : the state of the father and the state of the infants. The first remark we should here make is that the shorter the time between infection of the father and marriage, the more severe the symptoms of the offspring ; and conversely, the longer the interval the less severe ; till we arrive at those cases of Fournier's, where no ill consequences followed. Each case presents itself as an experiment, which begins with the inoculation of the father, and the question we have to consider is this : Is the experiment conducted under precisely the same conditions in all cases ? In the first place, is the poison the same in all cases ? There are good reasons for thinking that it is not ; and if you consider this matter carefully you will see what an immense number of cases we should require in order to have two exactly alike. Look then to the history of the father and all the results of inoculation in his case, with great care, before comparing them with the condition of the infant.

This suggests inquiries with which I will not venture to deal, but as a general rule you will find that the more severe the results of inoculation to the father, the more serious the symptoms in the offspring. Let us pass on now to the cases of S_2, where the mother showed symptoms of infection. They number ten, as compared with twenty in the last class, but these numbers do not by any means represent the proportion of mothers free from signs of infection as compared with those showing symptoms. Out of the large total of cases from which these histories have been drawn, all cases of evident maternal infection were noted, while histories of only 15 per cent. of the others were obtainable. In other words, if you analyse 300 cases of family syphilis after marriage, you will have about 10 in which there are signs of maternal infection, and 285 in which there are none.

Cases of S_2 *where the Mother showed Symptoms of Infection indicated by* S_{m2}.

Col. 1.	II.	III.	IV.	V.	VI.	VII.
1	4m	4y	Sm	Æt. 41, under M. de Meric, had secondaries at time of marriage.	Æt. 30, suffered severely from sore throat, &c., &c., but never local sores.	1. Born dead. 2. Syphilitic furuncles.
		18m	Sm_2	Reports himself healthy.	Sore throat and mouth soon after conception	Æt. 8m., snuffles, ears and nates sore. Appeared soon after birth.
	4y	8y	Sm_2	Æt. 36. sores and gonorr., no secondaries.	Æt. 25, eruption of skin when pregnant with this child.	1. Æt. 6, under Dr. D. Lee's three years ago. 2. Æt. 4½, syph. furuncles. 3. Healthy 2ys. old.
	2y	10y	Sm_2	Æt. 38, chancre, 2y. before marriage, no secondaries.	Æt. 34, sore throat frequent. No other symptoms.	1. Died, æt. 1w. 2. ,, ,, 3w. 3. ,, ,, 4y. 4. ,, ,, 6w. 5. ,, ,, 6w. Syph. cachexia.

Col. I.	II	III.	IV.	V.	VI.	VII.
	2-3y	3y	Sm_2	Æt. 30, chancre, no secondaries.	Æt. 28, sore throat occasionally during first pregnancy.	1. Born prem. dead. 2. Under treatment at Blackfriars when 6 m old for syph., æt. 3 m. pres. syph. cachexia.
	5y	9y	Sm_2	Æt. 39, chancre, no secondaries, gonorr. 3 y. ago.	Æt. 30, pains in thighs, ulcerated throat, sore gums, symptoms of syph. not very distinct.	1. Reported delicate. 2. Miscarraige months. 3. Reported to have rickets. 4. Delicate. 5. Æt. six m., brought for whooping cough. While under treatment had gummata on legs and thighs.
		2y	Sm_2	First husband healthy, 2nd husband reported of dissolute habits.	Good health till second marriage. No primary sym. General debility well marked.	Five children by first husband all healthy. First by second husband born dead at 7m. 2. Æt. 5w. Snuffles and syp. Cachexia.
	5y	7y	Sm^2	Æt. 35, salivated, now under Mr. Roger Bell with ulceration of tongue.	Æt. 35, no sym. until after birth of last child. Periostitis of left elbow	1. Died soon after birth. 2. do. do. 3. Alive and well. 4. do. do. 5. Æt. 15 m. with syph. furuncles on thighs and legs. An 8 m. child, sympt. developed after measles. 6. Æt. 10 w., similar to last.
	4y	4y	Sm_2	Æt. 30, under treatment 2-3 y. Had ulceration of tongue 6	Æt. 26, three weeks after marriage feverish attack, 3 m. later	1. Born dead, 7 m. 2. Born dead, 6½ m. 3. Born alive, 6½ m., lived 6 hrs.

Col. I.	II.	III.	IV.	V.	VI.	VII.
	4y	4y	Sm	m. before marriage.	eruption on arms, legs, and genitals. Under treatment 12 m., now well.	4. Æt. 14 w., under our treatment, lived 5 days.
	2y	11y	Sm$_2$	Æt. 30, under treatment for one month. Primary gonorr. no second.	Æt 27, 2 m. after marriage had sore throat, and 1 m. later eruption on chest leaving discolouration.	1. Miscarriage. 2. Born dead, Sm. 3. Lived 14m. An 8 m. child, which had erupt. at 2m. 4. Healthy. 5. Æt. 8m., has eczema.

If you had been personally engaged with me in the clinical observation of the cases in these tables, I have no doubt the same ideas or conclusions would have occurred to us, and similar thoughts suggested themselves upon points deserving of special study. Such questions as the following must occur to the mind: Why should a healthy child be born now and then in a family where those preceding and succeeding it were affected? Why should such a long period of time have intervened in some cases between the infection of the father and marriage, and then family troubles develope, while in others, where the period was one only of a few months, no troubles supervened? Why, in spite of active treatment of the father, has infection followed in some cases, while in others, where no treatment was used, there was no transmission?

We start with an experiment, the inoculation of the father; and we see a great variation in the consequences. We naturally ask ourselves, is this experiment, which we have been accustomed to regard as very much like that of vaccination, definite and constant in its character? The phenomena which succeed the experiment must decide this, and I need not say what conclusions they suggest. I must confess that up to the present time the result of observing the cases I have placed before you, and reflecting upon them has been to convince me that we are at present labouring under the pressure of erroneous ideas, which we must clear away from our minds before we can hope to observe and reason accurately. My object at present is to create in your minds a spirit of healthy scepticism and independence.

It remains for us to consider those particular cases which I referred to as being of great interest and as requiring a special symbol—$\frac{S_1}{S_2}$. We will defer their consideration till the next time we meet.

LECTURE II.

ON

THE TRANSMISSION OF SYPHILIS.

LET us consider now the cases symbolised by $\frac{S_1}{S_2}$

We denote by this expression that the mother has been infected, but that we are doubtful whether she has been infected primarily, or after conception, that is, through the fœtus.

I attach the greatest importance to these cases, because they throw light on the laws of transmission of syphilis in a way that no other cases do. What is remarkable about them is this—that the wife is infected soon after marriage, and suffers from local symptoms which are not of the character of so-called chancres, yet point to the conclusion that they must have resulted from direct infection. But when we inquire into the history of the father we find that he had so far recovered from local effects of infection at the time of marriage as to make it extremely doubtful whether the symptoms present in the wife may not be consequences of fœtal infection. There is another explanation to which I will refer presently. These cases are not common, so far as we may judge from the experiences of hospital practice. They are important for this reason—that they bridge over the interval which exists between the phenomena belonging to cases of S_1 and S_2; that is, between cases of primary and of fœtal infection.

If you look to Column II of the $\left(\frac{S_1}{S_2}\right)$ cases, you will see that the time which has passed since the primary infection of the father is less than in S_2 cases; that is to say, that the condition of the father is intermediate between the state of primary infection and the later state observed in S_2 cases. It seems to me that there is a strong argument herein, for the view that the syphilitic poison is transmitted in varying potentiality, and that the modifying force is Time, or at least that time has most influence in this respect. We see further, in looking down Column VII, that the results to the fœtus are also intermediate in severity between distinct cases of primary infection of the mother and those where the father has passed through a longer interval since he suffered from primary symptoms.

You may feel that instead of arriving at very distinct conclusions in regard to the transmission of syphilis, we are left in doubt as to the laws which regulate it; still you must allow that it is only the want of more data which prevents such satisfactory conclusions as we seek, from being possible. Such data are not likely to be collected by those who are satisfied with our present knowledge on this question. All our difficulties, I believe, can be overcome by patience and honesty, and I attach more importance to the latter than the former, for we see the most patient and industrious workers strangely influenced by ideas or theories which tempt them irresistibly to follow wrong directions and arrive at wrong results.

Col. I.	II.	III.	IV.	V.	VI.	VII.
1.	12m	12m	$\left(\frac{S_1}{S_2}\right)$	Æt. 20. Was under treatment from infection for 12m. up till marriage. Sore throat then.	Æt 20. Had local sores and sore throat soon after marriage.	1st Child; æt. 3 m. General dermatitis. Died at 5 m.
2.	4m	4y	$\left(\frac{S_1}{S_2}\right)$	Æt. 41. Chancres. Under Dr. de Meric. At time of marriage had secondaries.	Æt. 30. Local sores; severe constitutional symptoms.	1. Born dead. 2. Æt. 20 m. Furuncles.

Col. I.	II	II	IV.	V.	VI.	VII.
3.	4m	10y	$\left(\frac{S_1}{S_2}\right)$	Æt. 35. Infected 4 m. before marriage. Sores healed but recurred soon after marriage.	Æt. 30. Sores soon after marriage. Ulcerated throat, &c.	1. Born dead. 2. do. do. 3. Born alive, died æt 8 m. 4. Born alive, died æt. 9 m. 5. Alive. Had syph. dermatitis. 6. No symptoms. Seems healthy. 7. Present. Æt. 3 m. Snuffles. Specific derm.
4.	10m	15m	$\left(\frac{S_1}{S_2}\right)$	Æt. 24. Was under Mr. Royes Bell 5 y. before marriage with soft chancre. Had "gonorrhœa" 10m before marriage, with rash and sore throat, which continued till 4m after marriage.	Æt. 21. Tubercles of vulva "caught" from husband a few weeks before marriage.	1. Miscarriage at 5th week soon after marriage. 2. Æt. 1 m. General specific dermatitis. Snuffles, &c.
5.	6m	3y	$\left(\frac{S_1}{S_2}\right)$	Æt. 23. Chancre 6m. before marriage. Healed in 10 w. No secondaries.	Æt. 24. Local sores not definite. No other symptom.	1. Born dead. 2. do. do. 3. Present. Common syph. dermatitis.

I do not venture at present upon conclusions, and I only ask you to observe and reason for yourselves. Admit no authority, however exalted, without independent criticism, and demand facts rather than opinions.

Be careful above all things not to reason by analogy. It is a most dangerous method in such questions as we are considering, and has been the cause of more delusions than anything else.

Do not compare syphilis with small-pox or scarlatina; at least, do not let your minds be influenced by the idea that the same general principles are true of them all. Leave yourselves free at present from any such idea, for it may prove a detriment instead of an aid.

If we take the whole series of cases which present themselves to us for clinical study, we see a great variety of phenomena succeeding the transmission of syphilis in family life.

We begin with an experiment made upon the individual, with which is associated a certain series of phenomena of tolerably definite character. When that individual the male parent is brought into relation with the female we have certain phenomena resulting, and if these vary so greatly as they do, we naturally look to the conditions of the male parent for the explanation. If we can discover definite relations between the latter and the former we may establish at last a general principle or law which will explain the reasons of variation which at present puzzle and elude us

So far, however, as my own experience justifies an opinion, I am inclined to the view that the syphilitic poison may be transmitted from one individual to another in infinite degrees of potentiality, and that it is capable of infinite degrees of modification chiefly from the influence of time.

This view may be extended to all forms of infectious disease, and is one that may help in other lines of clinical study as much as in that of the one upon which we have been engaged, and it is for that reason that men like Trousseau felt so much interest in it.

In looking back over the cases we have been considering you observe that we have been dealing with five classes of phenomena which have followed the transmission of syphilis from the male to the female. We may arrange them in the following order—Cases of S_1, $\frac{S_1}{S_2}$ S_2; and then divide the last of these into those cases where the *mother and offspring* both suffered; where the *offspring alone* suffered; and lastly, where *neither* suffered. That is to say, we have five groups of phenomena to deal with. Do not imagine that we are justified in dividing them by any definite lines, however convenient it may be for certain purposes.

I see no reason to admit such a division as that of primary, secondary, and tertiary when dealing with the phenomena of syphilitic infection. It is a purely artificial system, hurtful to the scientific study of this difficult subject, and the sooner you free your minds from its influence the better. Let me also advise you to discard the idea of "blood contagion" in the transmission from mother to fœtus, or from the fœtus to the mother.

We know not enough to be certain on this point, though we do know enough to deny its probability.

Before bringing these lectures to a conclusion, I must say a few words on the subject of the history of syphilis. You might imagine that knowledge goes on steadily improving in matters of medical interest, as in most other branches of experimental science. But if you study carefully the history of syphilis during the last two centuries you will be astonished not simply at the little progress made during that long period, but at the retrograde movement which followed the teaching of John Hunter.

You can easily satisfy yourselves of the truth of this from the following sources: The work of Sennertus, translated by Care in 1676; the treatise of Swediaur published in England in 1784; and the work of Abraham Colles published in 1837.

I do not mean to assert that between the time of Sennertus and John Hunter there was no progress made, but I have no hesitation in saying that the knowledge possessed by our profession generally two centuries ago was more accurate and more extensive than at the beginning of this century. Knowledge theoretical and practical had retrograded, and ignorance and error had gained advantage.

You will find it convenient to fix your attention upon certain well-marked points of time in studying the history of such a subject as this; and I have mentioned Sennertus as deserving of special notice because he was the exponent of the most advanced views of his time, and the two centuries between then and now can be conveniently divided for purposes of historical inquiry. Swediaur and John Hunter were contemporaries, for John Hunter's work on the venereal disease was published in 1785, not many years before his death (1793).

It will be enough for our purpose then to take the four authors I have mentioned, viz., Sennertus (1676), Swediaur (1784, English edition), Hunter (1785), and Colles (1837).

Now the question to which our attention is specially limited is that of the transmission of syphilis from the parent to the offspring. We will dismiss entirely the question of direct infection of the female by the male, and confine our inquiry to the question what view did Sennertus, did Swediaur, Hunter, and Colles hold on the *possibility of transmission* from parent to offspring?

There is not the least doubt what each of them believed, for they have stated their opinions in the most distinct terms.

On page 48 of the translation of Sennertus, by H. Care, you may read as follows:—

"How many ways the contagion may be contracted.

"Sometimes it is hereditary.

". . . . . . . Sometimes it is transferred with the seed and menstruous fluid from the parent to the child, and the disease becomes hereditary. For when the blood out of which the seed is generated is infected and vicious, the like diseased seed is generated; also the mother's blood being impure with which the child is nourished, it pollutes that, which pollution afterwards, in those brought into the world, doth sooner or later show itself according to the lesser or greater strength of that virulency; which manner of original of this disease if any will deny to be properly by contagion because it is not by the contact of two bodies, viz., a sound and a sick, he may for all me: yet let him know this, that then that malignity in the body of the infant is not generated but from the infected parents by the seed; or the mother's blood is communicated to the offspring."

Here we have the view of Sennertus; but to make this complete you must go back to p. 45, and you will there find the great doctrine of transmission from the male parent to the offspring clearly and distinctly stated. The mode of infection by an ulcerated surface is contrasted with fœtal infection: "for though the seed in men is not wholly corrupt, when as we see many men infected in this disease do get children; yet it is altered and infected, from whence not only the issue contracts this evil, but also a woman may be infected by it."

You cannot but observe the spirit in which Sennertus wrote this, and what is indicated by the somewhat contemptuous way in which he treats those who did not believe in the view of hereditary transmission. Amongst them was Boerhaave, one of the great dogmatists of that day.

Now let us take the work of Swediaur, not the first edition published in England in 1784, but the French edition published in 1801, and for this reason, that in the interval between them, Hunter's work had appeared, and Swediaur makes special reference to it in his last edition: "I have added (he says, p. xii., ed. 4th, 1801) to the third English edition a short criticism of a work on the venereal disease by J. Hunter, which has been translated into French. The author is since dead, and his work, which contains many new and useful truths, with a number of errors and false directions for practice, founded on a vague empiricism, being almost forgotten in England, and not having been reprinted, I am excused from saying anything more about it."

A fair comparison of the actual knowledge possessed by Sennertus and Swediaur certainly leads one to the conclusion that during this century no real addition was made from direct observation to what was known by the former of transmitted syphilis. It is curious to observe the importance which Swediaur attached to an isolated case, which "appeared to place this question of hereditary syphilis beyond a doubt" On this one case he seems to have rested his belief in the possibility of transmission of the disease from the father to the offspring, though the former was free from any local sores, and had only constitutional symptoms. The whole of this chapter (p. 192-99) is worth careful study, as we can form a good opinion from it of what were the views of our profession in regard to this matter. It is absolutely necessary that we should do this before criticising John Hunter's work, because Hunter's views were not only original, but they were distinctly opposed to general opinion, the truth of which he condemned without sufficient respect and examination. There are two ways in which in our profession men seem inclined to strive for reputation; the one is to assert a claim to some original discovery, the other to deny some well-established truth. The latter is the easier method, and is for reasons we will not stay to examine, more likely to attract the ignorant and vulgar than the former. It is indeed strange that in the course of a century one case was all that was brought forward to prove the most important doctrine—that syphilis may be transmitted from the father to the offspring long after the possibility of direct infection from a local sore had ceased. This possibility Hunter denied. In order to show you most clearly what Hunter thought and taught, let us quote his own words from his work on the venereal disease:—"Many other modes of infection have been supposed, but I believe erroneously, such suppositions most probably having taken their rise from ignorance or deceit, two great sources of error in this disease" (p. 381).

"Hence it has been supposed that the testicles and vesiculæ seminales may be affected by the disease, that the semen may become venereal, may communicate the disease to others, and after impregnation, may even grow into a pocky child. *But all this is without foundation*" (p. 383).

"It is also supposed that a fœtus in the womb of a pocky mother may be infected by her. This I should doubt very much, both from what may be observed of the secretions and from finding that even the matter from such constitutional inflammation is not capable of communicating the disease" (p. 384).

"It has been supposed and asserted from observation that ulcers in the mouths of children from a constitutional disease, which constitutional disease has been supposed to be derived from the parent, would produce the same disease upon the nipples of women who had been suckled by them; that is, the children were contaminated either by their mother or father, having the disease in form of a lues venerea, of which I have endeavoured to *show the impossibility*" (p. 388).

You must admit after such a display of blind conceit as these quotations exhibit on the part of a man like John Hunter, we may be prepared to meet with a similar weakness in others of less capacity. If it were not for the extremely serious consequences to others of such erroneous doctrines I should hesitate to treat them with such severe criticism, but I feel under the circumstances more than justified in exposing the delusions into which Hunter led our profession, and the obstructions he thus offered to the progress of scientific inquiry.

You will understand better now why Colles felt bound to prepare the way for other views by the highly laudatory terms in which he wrote of John Hunter's work. All that Colles stated went to prove the very opposite of what John Hunter taught; but fifty years ago the name of John Hunter exerted such an influence over the mind of the profession that original observation and its expression were alike discouraged when it ventured to differ from Hunter's views.

The work of Colles is a great work, but its author laboured under a disadvantage common to many original observers; he was not sufficiently acquainted with what his predecessors had done in the same line of research. With this qualification I commend to you the careful study of this admirable work, and every part of it, as full of instruction and careful observation.

There is one remark with which I will conclude.

You will find the expression "Colles' law" sometimes used by authors as suggestive of some great discovery made by Colles. He discovered no law, or anything deserving the name of a law, as we use that term in physical science. He made an observation to this effect, that a syphilitic child never infects the nipple of its own mother. This we may take to be a fact, but to call it a law is to misuse a scientific term, and to retard the progress of inquiry. We may ask ourselves, how is this fact to be explained, that is a fair question; but to imagine that we add anything to the sum total of knowledge in the slightest degree by calling this fact a law is simply a delusive conceit.

My reasons for speaking thus strongly are that I hold ourselves justified in resenting in every way the least attempt to restrict that liberty and disposition to free inquiry which is the greatest protection and aid in the advancement of scientific knowledge.

LECTURE III.

ON THE

EARLIEST RECORD OF WHOOPING COUGH.

I PROPOSE to-day to review briefly the history of whooping-cough. We cannot say for certain when this malady first appeared in England. It prevailed and was well known to all classes long before physicians gave close attention to it, or recognised it as a distinct disease. If you seek to satisfy your minds as to when or how it originated, you will soon find yourselves lost in the obscurity which surrounds the origin of all diseases.

But in pursuing such an inquiry there is much of interest, and, what is more important, there is much of really practical and valuable knowledge to be gained, as I shall presently show you Of one fact you may be certain in regard to the history of whooping-cough, and that is, that we find no very definite account of it in this country till Dr. Willis gave his description in the year 1667. (*a*) We must not imagine that he made any discovery or claimed any distinction in this matter, for he only described with proper professional accuracy of detail what was well known to the common people, and had long before been well described by physicians in other countries. Nearly a century before the time of Willis, a learned French physician, De Baillou, better known by

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(*a*) Tract. Phys. Fol., trans. by S. P , 1684, c. 12, p. 92.

the Latinised name Ballonius, gave a very excellent account of whooping-cough; and I would advise you, if you feel an interest in historical research, to go back to the year 1578 as a fixed point from which to start, taking De Baillou as your guide. You will find his account in the edition of Stewart, published in 1734, p. 151, where he mentions whooping-cough in his Report of the year 1578, or rather the summer of that year, for the chapter is headed "Constitutio Æstiva."

The malady was evidently prevalent in Paris, and was known by the common name of the Quinta, or Quintana. To quote De Baillou's own words: "I have already spoken of the beginning of the summer. Towards its close almost the same diseases prevailed as before. It was a burning hot summer," and he goes on to say that fevers prevailed, "especially that common cough which is popularly called la Quinta, or Quintana, and of which I have already spoken. The symptoms of it are serious. The lungs are so irritated that, struggling as they do with every effort to rid themselves of what offends them, neither allow air to enter freely or are able freely to emit it. The patient seems to swell, and looks as if about to be strangled, from having some spirit sticking to his fauces. Why it should be popularly called Quinta is not certain. Some fancy that the word is formed by onamatopœia, that is, from the sound and ring which those emit who suffer from it. Others seek another origin, and think it is called Tussim quintanim, because it returns every five hours, and experience proves that this is true, for the attacks of coughing are often absent for a space of four or five hours, and then the paroxysm recurs, and is sometimes so violent that blood is forced from the mouth and nostrils. Very often, too, the bowel is everted." He concludes this notice with a remark to which I beg your attention: "I have not yet read any author who makes mention of this cough;" and later on at p. 160, he records the fact that many cases occurred in Paris at the same time as the particular case of which he gives an account, thus drawing the attention of his reader to the epidemic character, as he deemed it, of the disease.

But De Baillou did not rest satisfied with a simple record of his personal experience. He was an accomplished scholar, and a man of extensive literary culture; and the question of when the disease originated, and how far back it could be traced in the history of medicine, were subjects of his earnest inquiry. You will, therefore, find in his works the most valuable aid in historical research. We are accustomed to think highly of Willis, Sydenham, and other writers of our own country, but they seem to me to have learnt more than they confessed to from foreign sources.

Now let me tell you why the study of the history of whooping-cough is of real practical value. This disease has varied so much at different times that if we do not know its history we are certain to misunderstand its nature and dangerous character.- It is one of those diseases that may produce the most serious effects on the public health and be the cause of great mortality. A physician who knows not the conditions which favour its development and extension must be a dangerous adviser both to the individual and the community.

De Baillou believed that whooping-cough could be traced back to the time of Hippocrates, and it cannot be denied but that he gives good reasons for his belief. The name of Schenck (*a*) is often mentioned by those who make historical references on this subject. If you read what Schenck wrote, you will see that he had no claim to any kind of credit either as an original observer, or as a satisfactory compiler of other men's views. He does not pretend to have had any knowledge from personal observation of the malady, and refers to it by the name which the Parisians had given it of la Quinta or Quintana, describing it apparently only from hearsay evidence. It would thus appear that the people of Paris in the time of Ballonius—that is, more than three centuries ago—were the first to realise the fact that they were suffering from a somewhat new and special form of disease, whose characters were so distinct that they were compelled to give this disease a special name, and from that name I think there can be little doubt we adopted our term of chin-cough.

Now the question of most importance both historically and practically is not when and how the disease originated, but when and how it was first observed to possess

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(*a*) Lib. ii., p. 258. Fol. Francofurt, 1609.

distinctly an infectious nature. It seems to me that we gain very little by elaborate description of symptoms of such a disease as whooping-cough, or by long and tedious details for treatment, if that most important point is passed over, namely, that the disease is infectious, and that it is communicated from one individual to another by infection and by nothing else. The questions of symptoms and of treatment, interesting as they may be to physicians, and important to sufferers, are insignificant in comparison with the dangers which may and do arise from ignorance, doubt, or indifference in respect to the extremely infectious nature of whooping-cough. Even if the malady were less serious than measles or scarlatina, it would still be of great necessity to prevent its spread. But when we recognise the fact that in the case of very young children it is the most serious disease we have to contend with, we can easily appreciate the importance of studying the laws of its prevention

Now you will find that the minds of the physicians of the last and previous centuries were occupied with the idea that epidemic influences were largely concerned in the varying prevalence of such diseases as whooping-cough. As a proof of this we find Sydenham (*a*) following the example of Ballonius, though he lived and wrote a century later, for in his reports upon the years 1675—1680 we have his description of whooping-cough introduced.

But the essential nature of the disease was regarded by Willis as one specially affecting the nervous system, and as late as the end of the last century we find Cullen including whooping-cough in the same class with epilepsy and other nervous disorders. But Cullen knew that whooping-cough was infectious, and we see that professional views were beginning to alter about this time in regard to the etiology of the disease.

Let me caution you against the common error of attaching great and undue importance to the symptom of the whoop. Try and realise the fact that the whoop is a small part of the disease. The symptom, of course, is of great value in diagnosis, but it is of little value in aiding us to comprehend the real nature of the malady. Do not lose sight of this view of the matter, that the

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(*a*) Swan's Trans., pp. 238-89-90, 293.

whoop is only one of many symptoms of a disease affecting the whole system, and what is more, that it is by no means a constant symptom. No one believed more firmly in the invariable occurrence of the whoop when the disease was present than the late Sir Thomas Watson, till a very late period of his life; but he had an opportunity then of judging how far he was correct in a singular and very interesting manner. Let me conclude this lecture by reading a part of a letter which I received a few years ago from that most accomplished and justly venerated man. It furnishes at the same time an instance of what you will occasionally meet with, namely, the occurrence of a second attack in the same individual. This letter is dated Sept. 15, 1876, and Sir Thomas was staying with his son in the country:—

"Two of my son's four children went through that disorder some years ago, and this year the other two have had it, and from living in the same atmosphere I suppose the two elder ones, and even their father, have all had cough plainly spasmodic, without any whoop, however."

T. W. DANKS & Co., Printers, 71 Dean Street, Soho Square, W.